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peared a little short of its greatest elongation eastward; and again, on the 1st Sept. at 10 p.m., the satellite appeared a little beyond its greatest eastern elongation.

"During the late opposition of *Saturn*, although I have been very watchful of opportunities, I have been able to see it certainly but twice, viz. on the 16th October and 18th November.

"On the 16th Oct. at $8^h 35^m$ M. T. *Mimas* was, as nearly as I could estimate, exactly at its greatest western elongation, appearing to be five or six tenths of the length of the preceding arm of the ring distant from its extremity. This evening the satellite was better seen than on any other occasion.

"On the 18th Nov. at about 8½ hours, the satellite again appeared as nearly as possible in the same situation as on the 16th October. The power with which *Mimas* was generally best seen is a Coddington lens magnifying 567 times. The difference of visibility between *Mimas* and *Enceladus* is almost incomparable, the latter being instantly seen in my telescope, under all tolerable circumstances, when within 40° or 50° of its greatest elongation; whilst in any but the very finest circumstances *Mimas* is an object of great difficulty."

SATELLITES OF URANUS.*

Observations by Mr. Lassell.

"These observations are principally of the two brightest, those first observed by Sir W. Herschel, or I and II, with estimations of two others, presumed to be his inferior and middle satellites, or 1 and 2. I have not been able to obtain an undoubted observation of any satellite exterior to II.

"The observations are made in position and distance, like those of a double star. The positions are reckoned from the north point as zero, round by the *following* side.

"The results here presented are generally the means of two or three measures. The observations of the I and II, graphically projected, shew *apparently* elliptic orbits, having their transverse axes very nearly perpendicular to the plane of the ecliptic and the proportions of the axes, roughly, as 10 to 6.

"The results marked *e* are careful *estimations* only.

"As to the single observation of the satellite 2, there was unquestionably a point of light at the place indicated, which kept its relative position to the planet for about two hours. Moreover, that part of the sky in which *Uranus* was seen on Nov. 6 was carefully scrutinised on Nov. 8 without my being able to detect any stars in the places where the four satellites had been seen."

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* As a temporary nomenclature, we call the two first discovered satellites I and II, and the others 1, 2, 3, 4, reckoning from the planet.

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		1.		I.		2.		II.	
	G.M.T.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.
1848.	h	°		°				°	
Sept. 14	13·6	10 *e*		322 *e*				334·7	
27	9·8	326 *e*		160 *e*				345 *e*	
29	11·5	336 *e*		38 *e*				302·1	28″·98
Oct. 1	12·5	348 *e*		334·6	32″·8			213·4	27·8
2	10·5			307·7				192·1	
11	11·2			294·6	20·57			332·7	41·39
13	10·0			182 *e*				283·0	
15	11·3			133·3	26·27			196·8	33·44
16	9·5			77·6				180	29·15
17	11·0			9·4	27·37			166·1	46·38
Nov. 1	9·4			150				144·1	33·33
6	10·3	349 *e*	11″ *e*	262	20 *e*	80 *e*	10″ *e*	349	45 *e*
8				187·2				316·5	
9	9·5			284·1				163·8	

Note on the Satellites of Uranus. By M. Otto Struve.

The satellites of *Uranus* were first seen by Sir W. Herschel on Jan. 11, 1787, six years after his discovery of the primary planet. By a continued series of observations that year he established the undoubted existence and the approximate distance and periodic times of two *principal* satellites. In a second memoir (*Phil. Trans.* 1798) he announced the discovery of four new *supplementary* satellites; and in a third memoir (*Phil. Trans.* 1815) he presented the results of his observations to that date. The faintness of the objects observed and the mounting of his telescope* sufficiently account for discrepancies in Sir William's results. The measures in distance were, with his means, *exceedingly* difficult: hence we have only two such measures of the interior of the two principal satellites, and none at all of the supplementary satellites. There are, however, several measures of the exterior principal satellite, II.

In the memoir of 1788, Sir William gives 44″·23 as the greatest elongation of II from *Uranus* at his mean distance from the earth, and calculates the similar greatest elongation of I to be 33″, *i.e.* from the periodic time and Kepler's law. He considers these to be approximate only. In the memoir of 1815 he uses in round numbers, and for numerical convenience, 48″ and 36″ for the elongations; and he adds a list of selected observations from which more certain values may be deduced; but it does not appear that he performed this calculation. The other elements of the orbits of the two satellites are pretty exact; the position of the planet was very favourable for these determinations. In 1797 the apparent orbits of the satellites were nearly right lines.

As to the supplementary satellites and their orbits, it would appear

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* This was a 20-foot reflector with the *front view*, and Sir William's usual mounting.

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from both memoirs that Sir William did not affect to speak of them with confidence. Though he had no doubt of their existence generally, yet he avowed that "to determine their numbers and situation would probably require an increase of illuminating power." The precision with which their periodic times, &c. are stated by various authorities must not be supposed to rest on any certain foundation, but on Sir William's approximations and shrewd guesses.

Sir John Herschel directed his attention to the satellites of *Uranus* in 1828–1832, and the results are to be found in our *Memoirs*, vol. viii. The situation of *Uranus* was unfavourable, the planet having considerable southern declination. From his own observations, combined with those of his father, Sir John deduced the following periodic times:—

$$\text{I.} = 8^{d}\ 16^{h}\ 56^{m}\ 31^{s}\cdot3 \qquad \text{II.} = 13^{d}\ 11^{h}\ 7^{m}\ 12^{s}\cdot6\,;$$

but "of other satellites," he says, "than these two I have no evidence."

M. Lamont, Director of the Observatory of Munich, commenced his observations of the satellites of *Uranus* in 1837. His memoir is printed in our *Transactions*, vol. xi. They relate to the principal satellites, I and II. M. Lamont believes that he once saw what he takes to be 4, the most distant of the supplementary satellites.

The mass of *Uranus* has been found to be,—

$\frac{1}{20008}$, the Sun = 1, by Sir W. H.'s observations

$\frac{1}{24605}$ by M. Lamont

$\frac{1}{17918}$ by Bouvard, from the perturbations of other planets by *Uranus*.

M. Lamont does not consider his results as very certain, since I and II gave somewhat discordant results; but he is of opinion that Bouvard's mass is certainly too large.

The determination of the mass of *Uranus* has become of great and pressing interest since the theoretical researches of MM. Le Verrier and Adams into the apparently irregular motion of the planet, have led to the memorable discovery of *Neptune*.

If we suppose the two planets to be equal, it is clear that the effect of *Uranus* to perturb the motion of *Neptune* will be more sensible than the reciprocal effect of *Neptune* upon *Uranus*, on account of the greater distance of *Neptune* from the sun. The theory of *Neptune*, therefore, must continue imperfect until a correct value of the mass of *Uranus* can be assigned. With this object in view, M. Otto Struve has undertaken a series of micrometrical measurements of the two principal satellites, which he hopes to complete about the beginning of February 1848. But though the planet is now 5° or 6° north of the equator, and therefore far more favourably placed than when Sir J. Herschel pursued his investigations, still the observations are very difficult, requiring steady

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images and a perfectly transparent sky. In this unfavourable autumn only fourteen nights have hitherto been sufficiently fine. Generally speaking, the distances of II have been measured as satisfactorily as the angles of position, and only twice or thrice has the object been too faint to be well observed. The reduction is delayed until the series is completed.

When M. Struve had become familiarly acquainted with these two satellites, he turned his attention to their *supplementary* companions. After being twice deceived in measuring a small star instead of a satellite, he saw, on October 8, an object of which he made the following measures in position:—

	h m		
Oct. 8	11 16	Pulkowa M.T.	Position = 178°·7
	18		186·5
	35		178·8
	36		182·3
	42		176·7

An estimate of comparative distance made the distance = 14″·2.

These observations do not, indeed, prove the object to have been a satellite; but the difference between the first and last angles of position, if it had been a star, would, from the motion alone of *Uranus*, have been 9°. The following observations leave no doubt that a satellite was observed:—

1847.	Pulkowa M.T. h m	Position. °	No. Obs.	Pulkowa M.T. h m	Distance. ″	No. Obs.
Nov. 1	9 52	190·5	3	10 4	17·8	4
	10 23	194·0	3			
28	8 31	203·6	6	8 37	17·0	2
	9 48	202·1	4	8 52	16·7	2
Dec. 9	11 28	218·6	4	11 28	13·7	Estim.
10	5 53	180·1	4	6 3	17·0	4
	6 16	182·2	5			

Nov. 1. The satellite better seen than I and II, which are very near each other.

28. The satellite quite as visible as I.

Dec. 9. The images very good, but the observation not sure. The satellite seen by glimpses.

10. The satellite as well seen as II., but fainter than I.

The observations of Nov. 28 shew clearly enough that the object was a satellite: the *position* angle of a fixed star would have been altered 10°.

It may, at first sight, seem odd that *all* the observations are made on the *same side* of the planet. There are, however, many analogous cases among the satellites, if this should always occur. The satellites I and II, for instance, are found to alternate in brightness. There has been, however, no really fine observing night after Oct. 8, on which the satellite could have been observed on the other side.

The observations of this inferior satellite are not sufficiently

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numerous to yield an accurate orbit. It may, however, be assumed, pretty safely, that it has been observed near its greatest elongation, *i.e.* that the apparent semi-axis is between 17″ and 18″; in which case it must have a period somewhere about 3 or 4 days.

Now the satellite has, approximately at least, always been observed at the same part of its orbit, and hence the intervals of 24, 27, and 12 days, must, approximately, be multiples of the periodic time. The hypothesis of a period of 3 days, which first presents itself (or more accurately, from the observations of Oct. 8 and Dec. 10, $2^{d}\,23^{h}\,45^{m}$), M. Struve finds wholly incompatible with the observation of Dec. 9, and he prefers a period of 4 days (more accurately, $3^{d}\,22^{h}\,10^{m}$); which, admitting a slight error on Nov. 1, agrees equally well with the observations. This result also accords better with the presumed mass of the planet; for the first period and greatest distance, 17″·5, gives the mass $\frac{1}{15480}$, larger than Bouvard's, while the second period assigns $\frac{1}{26860}$ to the mass, somewhat less than Lamont's, but yet a tolerable approximation to it.

The difference between M. O. Struve's period for this satellite, and that of $5^{d}\,21^{h}\,25^{m}$, assigned to Sir W. Herschel's, is considered by M. O. Struve to be due to an erroneous estimate of the semi-axis. He has no doubt of the identity of the two objects.

After remarking that the foregoing observations completely prove the existence of Herschel's 1st supplementary satellite, M. Struve remarks that the visibility of this satellite depends less upon the aperture of the telescope employed than on the sharpness of the image shewn. All the telescopes, in his opinion, which will shew I and II, are *large* enough to shew 1, if they are *good* enough. It is not fainter than the other two, but, from its greater proximity to *Uranus*, requires better defining power and freedom from stray light.

M. O. Struve has not hitherto seen any other supplementary satellite, but he remarks that the position of *Uranus* is every year becoming more favourable for this inquiry; and there can be little doubt of their speedy rediscovery, so far as they actually exist.

MISS MITCHELL'S COMET.

Observations.

HAMBURG. (M. Rümker.)

1847.	Hamburg M.T. h m s	R.A. ° ′ ″	N.P.D. ° ′ ″
Dec. 11	18 6 34·0	227 55 12·6	98 44 40·5
17	18 7 6·5	229 25 22·0	93 44 17·6
18	17 34 54·4	40 11·7	92 55 49·9
19	18 3 24·7	229 56 5·0	92 5 1·7